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Interactive comment on "Hydrologic regimes drive nutrient export behavior in human impacted watersheds" *by* Galen Gorski and Margaret A. Zimmer

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REVIEW #1 In this paper, Gorski and Zimmer examine how examine nitrate dynamics in a series of nested basins in agricultural regions of Iowa. Specifically, they focus on the concentration-discharge (c-Q) relationships of nitrate in these rivers, using these relationships to make inferences about the source areas of nitrate delivered to the stream and the amount of in- and near-stream uptake that may modulate these c-Q relationships. In doing so, they divide the periods of analyses into stormflow and baseflow periods, predicated on recent work that shows that different c-Q relationships obtain in many rivers at high vs. low discharge. They find that stormflow periods were

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largely chemostatic (circum-zero c-Q slopes) while baseflow periods shifted between being chemodynamic and chemostatic depending on the season. They also found that increased ag infrastructure drove a more chemostatic basflow response. They make several interesting and plausible hypotheses as to why this might be the case. This is a good manuscript can be published with minor modifications. The paper provides an interesting case study of nitrate c-Q dynamics in a region that is very important for the Mississippi N budget. It is well-written and demonstrates a strong command of the extant literature. I would make the following general suggestions for improving an already-good manuscript.

Response: We thank the reviewer for their comments

- There are a LOT of comparisons made in this paper, with baseflow vs stormflow, seasons, and different metrics of land use all considered. All these comparisons, at some point, make it difficult to extract the main message from each section. I would encourage some revision to make these main points clearer.

Response: We thank the reviewer for this comment, we have attempted to clarify sections and make it clearer the main takeaway point from each. In addition, we have added language that was missing or unclear in some areas stating explicitly what we are comparing and why.

- The authors put a lot of stock in which variable (e.g. drainage density) was the most correlated with a particular c-Q metric, and they provide some interesting hypotheses/speculation about the ultimate causes of these correlations. They sometimes neglect other variables that were nearly as well correlated as the one they were focusing on, however, and they neglect to offer alternative hypotheses that might be as plausible. Given that there are only 5 watersheds in the study, there is likely a large degree of chance that determines which variables are most correlated with c-Q dynamics, especially if there is a high degree of correlation between predictors, which I imagine there is. I think they would do well to acknowledge some of these alternative hypothe-

ses. But... - With the exception of drainage density, the amount of variability in LULC, both total and within set buffers, is very low, and I would urge the authors to consider whether it is plausible that such small differences might plausibly drive the differences in c-Q patterns that they observed. I don't know one way or the other, but I think it's something to consider cerefully.

Response: We agree that with the number of watersheds analyzed it is difficult to draw strong mechanistic conclusions about the drivers of c-Q patterns. However, although the differences in land use are minimal, the difference in drainage density across the watersheds is substantial (as noted by the reviewer). For this reason, as well as previous work that has been done, we hypothesize that drainage infrastructure, at least in part, drives the c-Q patterns we observe.

Line comments: 52 My opinion: this puts too much stock in the ability of c-Q relationships to identify source areas. I think that c-Q relationships can help develop hypotheses about sources, but attributing a NO3 flux to a source area by c-Q relationship alone is a very tenous thread.

Response: We suspect that the reviewer may be referencing line 55: "An effective method for investigating contributing source zones within a watershed is the examination of the relationship between solute concentration and stream discharge (c-Q relationships)" We have amended the sentence to the following: "The examination of the relationship between solute concentration and discharge (c-Q relationships), in combination with other information about watershed structure and land use practices, can be an effective way to investigate contributing source zones within a watershed."

110 spelling "Glycine max"

Response: Thank you for catching this, the spelling has been corrected

115 It is my understanding that this area was drained in the late 1800s-early 1900s. https://www.desmoinesregister.com/story/opinion/columnists/2015/03/01/story-

pioneer-iowa-wetland-farmland/24212331/

Response: Yes, this area has a very interesting history, and in fact some of the drainage infrastructure is from the original draining of the area is still in place. We have updated text indicate that the drainage infrastructure was installed as early as the 1800s. Thank you for this informative article.

132 Is this volume-weighted mean concentration or just streight mean?

Response: This is just a standard average (not volume-weighted), we have added text to section 2.2 to specify. Standard average was used here to facilitate comparison to maximum contaminant levels and target concentrations.

135 spelling "Land Cover"

Response: Thank you for catching this, the spelling has been corrected

143 Why this absolute slope criterion rather than relative (either % flow change or criterion based on area-normalized Q?) Wont absolute criterion mean more storm detection in large rivers?

Response: We tried several different methods for selecting events from the record, and we found that the criteria did a reasonable job, although there are likely other ways to do this. Because the watersheds are in a similar area, the hydrographs had similar structures, however, if the analysis were to be expanded to other areas with significantly different hydrograph structures a different approach such as % flow change or area normalized Q might be more appropriate. We have added language to section 2.3 to clarify this.

143 I know USGS uses cfs, but please metricize discharge measurements, especially since you also use L volumes in the paper. Also 1e-4cfs/s is a nonintuitive quantity (is that like a tablespoon?) and mismatched to timescale of the data. can you put in mËĘ3dËĘ-2 units instead?

Response: We understand and agree that cfs is a non-intuitive unit and we debated whether to convert the discharge measurements to metric or not. We decided against it for two reasons 1) previous studies examining these watersheds reported Q in cfs, and in an effort to facilitate comparison we kept our records in cfs and 2) we are using USGS data, which is reported in cfs, and we want to connect our work to current USGS water quality efforts.

194 I would remove these statistical tests for a number of reasons: 1) It's inappropriate to model count data using a t-test because a t distribution is continuous 2) you're treating watersheds as independent observations in the t-test, but there is an obvious correlation structure given that the watersheds are nested. 3) I don't think the statustics are really necessary just to say "the count of spring and summer events and their precip totals were similar."

Response: The reporting of t-test results has been removed.

200 "+/- 3.07" Please state once what this variability estimate is (sd? I assume not SEM)

Response: This is a standard deviation; it has been added to the text

209 "groundwater flow paths with longer residence times, and more streambed-water interaction," The relevance of these factors may not be immediately apparent to a reader.

Response: We have added the following text to make the point more apparent to the reader: "...more streambed-water interaction which would allow for more nitrate processing in the subsurface and hyporheic zone."

205-215 - Suggest reworking this paragraph a bit. The central thrust of the argument isnt entirely clear.

Response: We have reworded the paragraph to make the main point more clear which is that watersheds with more drainage infrastructure show higher NO3 concentrations

which is consistent with previous findings.

221 - Another good cite for conceptual framework considering this idea: Wollheim et al. 2018 Biogeochemistry Response: Thank you, we have added that citation

279-294 - This might suggest that nonlinear power law fitting, instead of a linear fit in log-log space, might be a more robust approach to dealing with low Q anomalies, because in these periods, absolute residuals are small but log-residuals are huge.

Response: This is an interesting idea, and we agree that c-Q relationships should not be constrained to linear fits in log-log space without careful consideration of the data and the fitting procedure.

304 - mËĘ2 unit should be kmËĘ2

Response: Thank you for catching that, it has been fixed.

344 - I'm not sure I buy this explanation... given that you have multiple other predictors that are just sliiiightly less correlated than 100-m buffer crop % I think it's a stretch to focus on just one explanation. Could also be driven by drainage density, for instance.

Response: We agree that 100-m buffer crop is one of many factors that might drive this behavior, however because this does offer a plausible mechanism for the trend, we submit it as a reasonable explanation. Drainage density or topology could also be a contributing factor, and we have added language to acknowledge that as well.

Supplement: Figure S1: it looks like there is some linear interpolation going on over dates where there's no Q data e.g. MJF Dec 2017-Feb 2018. Please show gaps in Q data instead.

Response: Gaps are shown in this figure and we have done no interpolation. The period that the reviewer has identified is a low flow period, similar to those discussed in section 3.5.

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